DISCUSSION BEFORE THE WIRELESS SECTION, 2ND JANUARY, 1929.

Dr. R. L. Smith-Rose: The paper follows a trend which has been perceptible for some time in the policy of the chief engineer of the B.B.C., i.e. that broadcasting must utilize only the direct wave in order to give satisfactory service. To get these direct waves to the listener necessitates taking into account the attenuation of the waves due to the resistance of the earth, and that is a subject which, curiously enough, was neglected for some 20 years in the early development of wireless communication. In the early days of wireless it became so easy to transmit messages over long distances that the engineers responsible for the development of the subject neglected the study of attenuation; and it was not until the inception of broadcasting in about 1922 that attention was once more fixed upon the problem of attenuation of waves travelling over the ground. It was then realized how important it was to obtain a knowledge of the magnitude of losses due to the finite resistance of the earth. As the result of that appreciation, a number of papers in which the results of the study of attenuation have been set out have recently appeared. I should like to emphasize one point with regard to aerials, showing that one must not always expect theoretical results to work out in practice. In connection with some work we have recently been doing with Prof. Appleton, we have transmitted to him from the National Physical Laboratory, using a practically vertical aerial, from which one would not expect much radiation vertically upwards; yet at King's College, only 10 miles away, waves were received which had been deflected

from one of two ionized layers,* and the angles which those waves subtended with the vertical could not have been more than 2° in one case and 5° in the other case. The strength of the downcoming waves at those angles was comparable with the direct wave which was sent to King's College along the ground. Passing to the second portion of the paper, dealing with attenuation, we find a very interesting method for obtaining a whole family of attenuation curves for different wave-lengths, provided that we have one reliable curve as a basis. Incidentally I do not quite understand why the term " irreproachable attenuation curve " is stressed so much, because we are not provided with a country which is flat and uniform to an " irreproachable " degree. We should require such a country before we could apply some of the equations, which imply transmission over a perfectly uniform and flat country. We have to accept the earth as it is; and, as there are irregularities on it, I think that we can put up with some irregularities in the results obtained in transmitting over its surface. A very extensive study of attenuation has been carried out during the past few years by Mr. Barfield, who has obtained results which have been published in the *Journal*,† showing in great detail attenuation curves which have actually been measured in various directions on the London broadcasting transmitter. Yet, as far as I can gather from the paper, the results of his work do not appear to have been used by the authors. I suggest that they might have enhanced the value of their calculations considerably if they had made use of those results. The work was carried out under the auspices of the Radio Research Board, and it was not directed entirely to obtaining mere academic knowledge. We appreciated the practical aspect of the matter, and thought that the results would have been of some use to such organizations as the B.B.C. in connection with the problems presented in this paper. The results obtained by Mr. Barfield show quite definitely that attenuation varies in direction from the transmitter, and that the attenuation curves could be deduced from Sommerfeld's theory, provided the proper values of conductivity were inserted. Those values do not vary to any greater extent than the values obtained in the paper. I suppose that the real problem before the broadcasting engineer is that he must supply a minimum field strength over a given area. It does not matter whether the field strength is 100 per cent greater than that minimum at some places, since the listener will be satisfied. It does matter, however, if someone within that area is getting a smaller field strength, so that probably it is the worst conditions, corresponding to the lowest value of earth conductivity, which have to be taken into account, although I am not quite sure that an average value would not be more feasible to work upon. I do submit, therefore, that if more attention had been given to the results which have already been published, they would have been useful in connection with this problem. The experiments described in the paper do not give results of a high order of accuracy, but I would not criticize this feature as I am well aware, from practical experience, of the difficulties accompanying measurements of attenuation. The authors appreciate this matter, because on page 61 they say " It was seen that the points do not lie by any means on a smooth curve. Intelligent interpolation, however, gave a family of smooth curves for two directions, as shown in Fig. 12." I was puzzled at first as to what " intelligent interpolation " meant, but when I read a little further on (page 62) that they were using that curve and an equation from Sommerfeld's theory to calculate the conductivity for two different wave-lengths, and that the value of σ obtained was identical in each case, then I realized what great intelligence had been put into the interpolation. The order of error in the results is shown, incidentally, by the curve " B " in Fig. 13, in which case some of the points appear to be as much as 50 per cent off the curve. I stress the point, therefore, that although this curve is taken as a basic calculation, it is not irreproachable, and I think that it is not superior to similar curves which already exist.

Mr. R. M. Wilmotte: I wish first to refer to the question of the term " effective height," with which term the authors do not agree. I think that most people disapprove of this term; it is misleading, but I do not think that the authors have obtained a satisfactory solution of the problem. First of all, from simple measurements of field strength one is led into very great difficulties. Even at 5 wave-lengths, as is mentioned here, there are apparently considerable discrepancies in the measurements obtained. I might mention a paper by Ratcliffe and Barnett,* in which they found a big variation, which, up to now, has not been very satisfactorily explained. I believe that it is due to the actual size of the aerials, the upper portion interfering with some lower portion. But if we depend on the value of the effective height as determined by measurement, it is necessary either to find out the reasons for these discrepancies or to refrain from making any distinction, and so leave it to posterity to make such a definition when the theory is further cleared up. The use of the term " effective current " was recently suggested to me, but I do not think that it is very suitable, because the effective current is not a property of the aerial alone and is, in that respect, unsatisfactory. Possibly some such term as " radiation constant " or " radiation factor " or " radiation strength of the aerial " might be more suitable; but I think that the current should not be an integral part of the definition. In any case the direction for which the value is applied should be stated as well as the point of the antenna system where the current is measured, for the term becomes meaningless in the case of directive or even partially directive systems. In a recent paper † I used the term " directive efficiency " for an aerial, meaning the ratio of the energy transmitted over the whole space by the aerial to the energy transmitted in the direction required; and I think that term is useful. In other words, if E_0 is the field strength in the direction required at the distance r, the directive efficiency is $E_0^2 r^2 / \int\int E^2 dA$. Referring to page 56, it is interesting to find that the experimental field strength from a $\frac{1}{4}\lambda$ aerial is

* See *Nature*, 1927, vol. 120, p. 330.
† *Journal I.E.E.*, 1928, vol. 66, p. 204; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 14.

* *Proceedings of the Cambridge Philosophical Society*, 1926, vol. 23, p. 228.
† *Journal I.E.E.*, 1928, vol. 66, p. 955; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 188.

half that from a $\frac{1}{2}\lambda$ aerial. In other words, there is a constant factor in error in measuring effective heights at a given point, for I presume that those ratios were referred to some given point. If this was carried out at several points it would be of interest to know to what degree of accuracy the values agreed with one another. Mr. Munro made some measurements, in connection with a paper by Mr. Barfield * some time ago, on a T aerial in which he increased the length of the horizontal portion. I calculated how the effective height should vary with the increasing length of the horizontal part; and the correspondence between these calculations and the actual measurements was quite astounding. It was far closer than the experimental accuracy to which Mr. Barfield thought the experiments were true. As a matter of fact, that curve has been published in the *Journal*.† The interesting point is that it shows that the current in the two quarter-waves forming the half-wave must be in phase. If they were not in phase, one would not obtain this doubling of field strength when the aerial is doubled in length. The main conclusion reached in the paper is not new, in so far as it was already known that, if one wanted a good directivity in a given direction, say horizontally, one had to employ a high aerial. I do not mean that it was experimentally known, but it was certainly theoretically known. In the recent paper which I have already mentioned, I showed mathematically that if one wanted a beam in a given direction the best way of setting the aerial was to spread it—preferably in a line perpendicular to the direction in which the beam was required. In this case when we require the beam in all directions horizontally, the only way to obtain the condition deduced theoretically is to spread it in a vertical direction. That is the conclusion to which the authors have come. The authors use $\frac{1}{4}\lambda$ and $\frac{1}{2}\lambda$ aerials. It would be better if they could get a more uniform distribution of the current all along the aerial. That might be done by cutting, by means of phasing-coil arrangements, those parts of the aerial in which the current was of small value, so that one could have a little more effective height than if the current were sinusoidal, as occurs in the case of a perfectly straight wire.

Mr. R. H. Barfield: My chief interest centres on the attenuation results obtained by the authors, and I am glad to see that, in measurements in the country north of London, they found the same value for the conductivity of the earth as I obtained in my experiments in the same area published in the *Journal* ‡ about a year ago. The present paper is supplementary to mine in that it contains measurements on a great number of different wave-lengths in one or two directions, whereas I measured the attenuation in a number of different directions on one or two wave-lengths. I infer that the authors' uncertainty about the effect of trees refers only to the evidence of their own experiments. The evidence on this point contained in my paper already referred to is, I think, very nearly conclusive, and I will here take the opportunity of summing it up. The attenuation (that is the departure from a constant " intensity × distance " product) in the country south of London is from 4 to 5 times as great (at, say, 100 km) as the attenuation in the country to the north of London at the same distance. This result, by the nature of the experiments, is entirely independent of any " town effect " or transmitter characteristic. There are only two possible explanations of this fact. The first is that the earth in the southern Counties has an average conductivity of about one-third of that in the northern Home Counties, and considerably below the lowest value yet observed for any kind of soil in England. The second is that objects on the surface play an important part in determining attenuation and that their density is greater in the south than in the north. Of these the first (as I showed in my paper) has no experimental evidence at all to support it; while on the contrary an increasing number of conductivity measurements made recently show that such a state of affairs is very improbable. We find, in fact, that almost every kind of soil has a conductivity from 3 to 5 times as great as that which on this view would be required for the southern districts. In support of the second and only alternative, we have the fact that there is undoubtedly more vegetation in the southern Counties. While a calculation based on observations made on individual trees shows that in this type of country tree absorption should actually predominate over earth absorption, other surface objects apart from vegetation can almost certainly be left out of consideration. The tree or vegetation hypothesis therefore holds the field without a rival, and surely cannot be described as premature. But we now have further evidence. Following out a suggestion made by Prof. E. V. Appleton* (in the discussion on my paper) we have carried out two sets of experiments. The first was to measure the energy absorption due to a given tree, and this we found definitely to be greater by about 30 per cent in the summer than in the winter. We did that with two typical kinds of trees. Secondly, during the last year, at the Radio Research Station at Slough, Mr. Munro has been very carefully measuring the field strength of the Bournemouth broadcasting station during the whole year week by week, and we find that we can detect a definite seasonal variation of attenuation, which, taken in conjunction with the first experiments, we naturally ascribe to the alteration of the intervening vegetation with the seasons. These results we hope to publish in full in due course. The value of the curves in Fig. 10 for forecasting is limited if one does not know the conductivity of the ground over which one is forecasting. Is one to take the limiting values of σ given by the authors? If so, the limits of the forecast will be very wide indeed. If not, we must take the value of σ represented by the curves (Fig. 10) themselves. Supposing we use those curves this way to forecast the attenuation of field strength over a county like Surrey or Sussex, we should find at distances of, say, 100 km that the forecast value is 4 or 5 times too large. We might well forecast a " B " area where a " C " area would be obtained in practice. Hence

* *Journal I.E.E.*, 1928, vol. 66, p. 204; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 14.
† *Journal I.E.E.*, 1928, vol. 66, p. 215; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 25.
‡ *Journal I.E.E.*, 1928, vol. 66, p. 204; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 14.

* *Journal I.E.E.*, 1928, vol. 66, p. 215; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 25.

in addition to those curves some guide as to how to estimate the conductivity is very necessary. The experiments and conclusions in my paper provide a first approximation to such a guide. If, however, the B.B.C. repeat their experiments over wooded country, as I hope they will, we shall undoubtedly get further valuable information.

Mr. E. H. Shaughnessy: I imagine that Captain Eckersley will make good use of this paper in his negotiations with other broadcasting authorities, and if he can prove that a 200-metre wave is quite satisfactory, I think that he will do valuable work. Apart from that, I am glad to see that the engineers of the B.B.C. are taking this matter very seriously. An efficient design of aerial may save very large sums of money, as it will be possible to employ less power.

Mr. F. H. Amis: The paper is evidently a further step on the part of the B.B.C. to improve their already excellent service by gaining such information as will enable them to utilize their transmitters to the best advantage, and by endeavouring to reduce interference. The paper serves a useful purpose in emphasizing the fact that the higher efficiencies which it is economically possible to obtain with the shorter wave-lengths do, to a certain extent, counteract the disadvantages connected with the use of these wave-lengths. On page 52 the authors infer that by endeavouring to obtain a maximum ratio of radiation to total resistance, engineers have been endeavouring to obtain the greatest amount of radiated power irrespective of whether the power is radiated at ground-level or at an angle. It is, I think, generally appreciated that the only way to gain accurate information as to the effective height of an antenna is by measurements of field strength. Such field measurements are necessarily carried out at ground-level and therefore the radiation resistance is calculated on the above basis, from which it surely follows that the figure obtained for efficiency is in terms of the maximum power radiated at ground-level and the total power put into the antenna. This appears to be the only way of obtaining information as to whether a radio transmitting station is serving an area to the limit of its capabilities. As pointed out by the authors, the higher type of antenna certainly does yield a relatively greater field strength than does the normal antenna of $\frac{1}{4}\lambda$ or less. The authors' claims are well supported by some experiments made in Sweden, where a change from a good vertical $\frac{1}{4}\lambda$ antenna to a $\frac{1}{2}\lambda$ T antenna produced an average increase in field strength of about 25 per cent. On changing the antenna to a vertical $\frac{1}{2}\lambda$ type, however, the increase of field strength over that obtained with the $\frac{1}{4}\lambda$ antenna was of the order of 70 to 80 per cent. A wave-length of approximately 260 m was used for these tests and the total input power to the antenna was the same in all cases. One is frequently faced with the problem of increasing the efficiency of an antenna which is supported by masts only high enough to support a $\frac{1}{4}\lambda$ antenna. In such a case, the increase which can be obtained obviously depends upon the amount of dead-loss power. This dead-loss power is more or less proportional to the amount of absorbing mass and, as such absorbing masses are generally near the base of the antenna, to the amount of current at the base. This is illustrated by some measurements in which I took part a few weeks ago in Sweden. In this case the masts were just high enough to support the $\frac{1}{4}\lambda$ vertical antenna of a station operating on a wave-length of approximately 260 m. By turning this into a $\frac{1}{2}\lambda$ T antenna an average increase in field strength of about 80 per cent was obtained. It is thought that the greater part, at least, of this increase was due to getting the maximum current higher up the antenna and thereby reducing considerably the amount of absorption due to the building, etc. It will be noted that in the earlier experiments made in Sweden the above change only yielded an increase in field strength of 25 per cent. The difference is thought to be due to the difference in the amount of absorbing mass near the bottom of the antenna in each case. A further increase of 15 per cent in field-strength from the antenna which had already yielded 80 per cent increase when turned into a $\frac{1}{2}\lambda$ T antenna was obtained on turning it into a multiple tuned antenna with three down-leads, and it is interesting to note that no directional effects resulted. The only other point I want to mention is that I was rather disappointed to see that the tuned-mast idea had been turned down without a trial.

Lieut.-Col. A. G. Lee: The first section of the paper deals with the design of a transmitting aerial from two points of view, the first being to reduce the indirect ray with the object of reducing the long-distance range of the station, and thus temper its properties of interfering with other broadcasting stations, and the second being to design the aerial for a maximum horizontal radiation, this being the only portion which is effective in a local broadcasting system. With regard to the first object, a reference to Fig. 7 shows that at 300 miles' distance the calculated indirect ray for a $\frac{1}{2}\lambda$ aerial is 0·7 of that given by a $\frac{1}{4}\lambda$ aerial, an order of difference which I am afraid would have no appreciable effect on the European broadcasting difficulties. Further, the experimental results on page 58 do not corroborate the calculations, even in regard to the fading effects at moderate distances. I therefore think that no case has been made out for the high mast, with its enormous cost, as a solution of the interference problem. On the second point, a reference to the table on page 57, giving the experimental results, shows that, employing the same power for a $\frac{1}{4}\lambda$ and a $\frac{1}{2}\lambda$ aerial, the relative field strengths at a given distance are 1 and 1·26 respectively. The difference in mast expenditure to produce this result is probably eight times greater for the $\frac{1}{2}\lambda$ than for the $\frac{1}{4}\lambda$ aerial, so that the problem resolves itself into a simple economic one when all the data are known. The annual charges on mast expenditure persist throughout the year, as their name implies, whereas the expenditure on power is only incurred while the station is in operation. The difference in plant expenditure due to the requirement of increased input power in the $\frac{1}{4}\lambda$ aerial case is small but easily calculated. I should be surprised to learn that the 800-ft. masts were justified from this point of view.

Mr. T. McGrath: During the experiments did the authors find that transmission was better when the aerial was at an angle to the vertical, especially at that angle in line with the vertical component of the earths'

magnetism, or in the plane of the dip or inclination? The possibility of the earth's magnetism tending to increase the service area by assisting the field-strength E might have been considered. Secondly, I should like to know whether the humidity of the atmosphere during the tests affected the transmission. I think that humidity plays an important part, and that in the design of a transmitting aerial care should be taken that the effect of humidity should be eliminated, if possible, and that insulators should be made with a surface which will not retain moisture.

Mr. K. Sreenivasan (*communicated*): I am not able to follow the authors in what they say regarding the surge impedance. When a straight wire is rearranged in any manner, its L/C ratio is bound to change, so that the chances of securing the original ratio cannot be very great. In the measurement of the constants of the aerial above and below the fundamental frequency, the method of added reactance and resistance of known values is possible, although considerable care has to be exercised during the experiment, as shown by J. K. Catterson-Smith.* As a result of their experiment, can the authors state whether the ratios of the current at the base of the aerial are equal to the corresponding ratios of the measured field intensity at a given distance, everything else remaining the same and the observations being made quickly one after another? In view of the difficulties and the complicated arrangement needed in the balloon experiment, it would appear that reasonably tangible results would be obtained by stretching the aerial and its accompaniment horizontally. It would then be easier to support the phasing coils and introduce extra $\frac{1}{2}\lambda$ units without too much trouble, in addition to other minor advantages. The horizontal polar diagram would then roughly correspond to the vertical diagram of the actual aerial, and the effect of the addition of phasing coils and additional lengths of aerials would give useful information. The distribution of current along different parts of the aerial could also be studied in this manner. I do not know what effect the proximity of the earth would have, but I am inclined to think that it could be allowed for. The measurements made on the London transmitter of the B.B.C. by Mr. Barfield, and the remarks of the authors, lead one to think that a solution of the mast problem lies in using wood. The absence of white ants, and the general climatic conditions of England, appear to be favourable to wooden masts. As masts are not built to last for more than a very few decades, the only objection to such masts seems to be the risk of fire. Care being taken to have the minimum amount of metal in the structure in order to avoid eddy-current heating, a good case can be made out for wood, especially when the transmitter is not in the middle of a busy city but somewhere out in the open. The 220-metre trellis masts of San Paolo form a good instance. There are two points in connection with the $\frac{1}{2}\lambda$ and $\frac{1}{4}\lambda$ aerials on which I should like some information. At the top of the first column of page 56, ambiguity about the effective height of aerials in spite of careful check measurements is emphasized. I wonder whether this slight discrepancy partakes of the nature of the end-effect familiar in sound pipes, where the resonant wave-length is slightly greater than four times the length of the sounding tube. In view of the difficulties of introducing phasing coils between two $\frac{1}{2}\lambda$ aerials at broadcast frequencies, is it not possible to connect two or three similar non-radiating units in series with each other? The difficulties of supporting these and of any inductive effect they might have on the aerial lengths would, I venture to think, be reduced. More extensive data are required before confidence can be placed in the method of deriving field strengths at different wave-lengths from observations at one wave-length. Is the method adopted here based on the proportionality of λ^2/d^2 as indicated by the example given?

* *Journal of the Indian Institute of Science*, 1926, p. 21.

Captain P. P. Eckersley and **Messrs. T. L. Eckersley** and **H. L. Kirke** (*in reply*): There appears to be a feeling in the minds of some who have taken part in the discussion that nothing in the paper is new, but that the proofs of the correctness of supposedly long-advanced theories set out in the paper are open to doubt. The British Broadcasting Corporation, however, undertook the work because there seemed to be a good deal of theory not established in practice. For instance, it is doubtful if full and sufficient proof of the relative effectiveness of $\frac{1}{2}\lambda$ and $\frac{1}{4}\lambda$ aerials existed before the so-called "balloon experiments." Further, the curves in Figs. 12, 16, 18 and 19 have never been published, as far as we are aware, in their present form—a form useful to engineers. This will answer Dr. Smith-Rose where he says that he cannot see their superiority to others. Mr. Barfield established that attenuation was different in different directions; which means, if the Sommerfeld theory is accepted, that earth resistivity varies in different parts of the home counties. We show in the paper that, given a certain earth resistance, actual service area can be accurately predicted knowing the number of kilowatts in the aerial, the mast height, wave-length, etc. Mr. Barfield obtained results on only one wave-length.

Dr. Smith-Rose does not fully understand the term "irreproachable attenuation curve." We mean by this a curve taken along a certain line, so carefully that it gives a value (or several values) of σ for one wave-length along that line, and only along that line. If this is done, the engineer who finally is responsible for practical results is able, by means of the paper and the data given therein, to forecast his service along that line for any other wave-length in the broadcasting band. By taking irreproachable curves in many directions the service area at any wave-length and with any mast height can be sufficiently forecast. From an engineering point of view it was therefore essential to supplement Mr. Barfield's valuable and interesting work by obtaining data useful to engineers. Mr. Barfield proved that, given some radiation (in his case from the roof of a large shop), then attenuation was different in different directions. This paper proves that, given the knowledge of certain quantities, service area can be forecast reasonably accurately.

With regard to the so-called vegetation theory, we submit that this is only proved to the extent that it shows that attenuation increases where there are large agglomerations of trees. The point which we wish to

emphasize is that few trees on one kind of earth might give the same attenuation as many trees on a different kind of earth. There are no data to allow the calculation of attenuation in terms of trees, different kinds of earth, height and extent of hills, etc. We must to-day find attenuation curves experimentally and calculate σ, which finally allows us to predict service area on any wave-length over a given terrain. In some recent measurements it was proved that a certain mountainous but treeless ground gave much greater apparent attenuation than flat but wooded ground in the south of England. In short, the vegetation theory, if it merely says that forests on a given type of ground give greater attenuation, is proved. If the vegetation theory is said to be so complete that it enables the broadcasting engineer to separate by calculation tree losses from all other losses, then the theory is by no means proved.

With reference to Dr. Smith-Rose's remarks, we should like to point out that the "intelligent interpolation" only appeared by chance to be so intelligent. The curves were, in fact, first drawn from experimental points, the value of σ being thereafter calculated.

The part of the paper most susceptible to criticism is that dealing with fading. We agree therefore with much of Col. Lee's reasoned criticism. No case has been made out that the $\frac{1}{2}\lambda$ aerial will produce daylight conditions at night, or that the curves of Fig. 7 are verified. The first part of the paper, however, sets out a policy, namely, that of relying upon the direct-ray service for broadcasting. We have proved that the $\frac{1}{2}\lambda$ aerial increases the ground ray, so, presumably, it decreases the space ray. This is to the good, but from the experimental fading tests the degree of effectiveness in reducing the interference-producing qualities of the space ray at long distances is negligible.

Col. Lee thinks that the question is purely economic and asks whether it is better to spend money on high masts or on high power. But this is not the whole of the question because, although our experiments do not give an indication of much reduction of the space ray at large distances, there is sure evidence of its reduction at shorter ranges. At any rate it is certain that the higher-angle radiations must have been reduced because the ground ray has been increased. Power does not determine the limits of pure service area because of the interference by indirect ray with direct ray. This interference is independent of the power, but is directly dependent upon the ratios of these quantities. The use of higher masts would therefore appear to be justified as a means of increasing the service area, which would otherwise be limited by fading at its outer boundaries.

The solution of the pure economic problem cannot be written down in general terms because there are so many variables, e.g. required service area, value of σ, cost of power, cost of masts, etc. It can be said, however, that the higher mast (considered from the purely economic standpoint put forward by Col. Lee) is more and more valuable as the power of the station is increased. An 800-ft. mast costs the same for a 100-watt as for a 100-kilowatt station, but the power costs are a thousand (or more) times greater in the latter case.

We agree with Mr. Wilmotte that "effective current" is, perhaps, not a good term; we did not recommend it. The ratio of theoretical to measured effective height for the two aerials was certainly, as Mr. Wilmotte supposes, derived from measurements taken at the same points. We still maintain that effective height should be measured for each and every aerial when it has a very practical meaning for the wireless engineer. While discussing this question we should like to point out that the figures given by Mr. Amis may be a little misleading, as Captain Eckersley has the authority of Mr. Lemoine, who is in charge of the technical side of Swedish broadcasting, for saying that they have not been quite correctly quoted. No doubt, however, in general they confirm much of our work.

With regard to surge impedance and Mr. Sreenivasan's criticism, we would point out that, in order to obtain correct results with Franklin aerials, it is theoretically necessary that the phasing units have the same surge impedance as the active aerials. If the surge impedances differ, reflections will occur and the current in the various $\frac{1}{2}\lambda$ aerials will not be the same.

As stated by Mr. Sreenivasan, the L/C ratio of a wire changes when the wire is rearranged in any manner. Under practical conditions, however, the change is small and can, in fact, be compensated to some extent by a change in gauge of wire.

When comparing $\frac{1}{2}\lambda$ and $\frac{1}{4}\lambda$ aerials the current at the bases of the aerials cannot be used for comparison. It is necessary to consider the current at the current antinode. This is expressed as I in the formula $E = 377hI/\lambda$.

With reference to the measurement of aerial constants, the apparent impedance at the base of the aerial gives sufficient data for calculation when we are considering pure $\frac{1}{2}\lambda$ and $\frac{1}{4}\lambda$ aerials. The value of $\sqrt{(L/C)}$ can be found for a $\frac{1}{2}\lambda$ aerial from $V/I = \sqrt{(L/C)}$, where V = voltage between the base of the aerial and earth, and I = current at the current antinode (see Figs. 8 and 9).

Mr. Sreenivasan suggests wooden masts. We fear that this suggestion is impracticable owing to the high cost and the fire risk. It is, however, only fair to say that the German administration uses 200-ft. wooden towers. One still doubts the practicability of 600-ft. to 800-ft. wooden towers.

Answering Mr. McGrath, it did not occur to us to go to the expense of holding a flexible wire some hundreds of metres long "in the plane of the dip or inclination" to see if the earth's magnetism would increase the service area. Further, there was little time to investigate the question of humidity and its effect upon insulators, because the balloon did not ascend during very damp weather. As far as we are aware, however, porcelain insulators as used in nearly every wireless station in the world are not liable to absorb moisture and even when wet on the surface do not, when properly designed, greatly influence the radiation constants of an aerial.

It should be mentioned that Dr. Balthasar van der Pol, jun., communicated to the Physical Society, in 1917, a paper on the wave-lengths and radiation of loaded antennæ which covered much of the subject dealt with in the first section of our paper. The interested reader is referred to the paper above mentioned.

The assumption in this and other theoretical papers has been that the earth is perfectly conducting. It is interesting to notice that the results of experiments mentioned in our paper prove that the radiation from an aerial is the same as that which would occur if the earth were perfectly conducting, and that the effect of the imperfect conductivity of the earth can be expressed as a constant additional aerial resistance.

We think it would be of interest to quote extracts from the translation of a letter to one of the authors received from Prof. Dr. Ing. A. Meissner of the Telefunken Gesellschaft: " The paper's special value for us is not only that it has shown without doubt that the best solution of the broadcast antenna question is to have the mast as high as possible, but also because the field strength at different distances from the transmitter can be determined in advance for any single wave out of the complex curve material. With regard to the masts and the question of antenna height in particular, an experimental comparison of $\frac{1}{4}\lambda$ and $\frac{1}{2}\lambda$ antennæ for the broadcast range was lacking. And here it was exactly your balloon experiment that we needed. The principle of employing masts as high as possible was actually introduced into broadcast practice by the Telefunken themselves, but rather because of conviction of the correctness of the principle than on the basis of deciding experiments. The experiments which Telefunken have carried out here in March 1926, and later in conjunction with the Telegraph-Technical authorities, suffered under the disadvantage that there was not such a perfect method then available for measuring field strength as the B.B.C. now have. That it was not possible to employ antennæ shortened to $\frac{1}{2}\lambda$ for the broadcast range before 1926 was due to the fact that transmitters with separate control only came into use for broadcasting after this time. Then it was recognized that such an antenna, the tension nodes of which were at the earthing-point—the ammeter in the earth lead showing therefore no current—could be perfectly excited by a transmitter if the latter was separately controlled. From the intermediate-circuit current and the anode current of the last valve-stage, one could recognize that the antenna had taken up the full amount of energy. The best experimental verification of the point of view that you have put forward in your work is in the results which have been obtained with the Budapest broadcast transmitter, which was completed 3 or 4 years ago and which is well known on account of its great range. Here there are two masts, each of 150 m height. The fundamental wave is 555·6 m. The natural period of the antenna is 930, and the shortening 400 cm. According to detailed reports of the Hungarian post and telegraph authorities, with 15 kW continuous-wave loading in the antennæ the field strength at a distance of 50 km from the transmitter is up to 30 mV per metre, at 100 km up to 10 mV per metre, and at 150 km from 1 to 5 mV per metre. It is stated that fading occurs first at 150 km. A similar antenna is to be built immediately in Oslo; two 150-m masts with a power wave of 496·7 m. With this wave-length one can hardly go higher than 150 m. One is limited in the choice of height, as the natural period of the insulated mast should not come in the region of the transmitting wave-length. In this case the field strength loses its symmetry round the transmitter. The field strength is stronger in the direction of the mast, and weaker in the direction at right angles to it.* In order to have a technically practicable mast, one is forced to the compromise of making the mast smaller than $\frac{1}{2}\lambda$, and the antenna at the top in the form of a T, so that the centre of gravity of the upper half of the $\frac{1}{2}\lambda$ antenna still lies at nearly the same height that it would do if the vertical antenna wire were contined upwards to a length of $\frac{1}{2}\lambda$. Since 1927 the German Telegraph Technical Minister, Mr. W. Schäffer, has also introduced into German broadcast transmitters the principle of making the fundamental wave equal to 0·6 of the natural frequency of the antenna. Here free-standing masts of 100 m height are employed. With regard to the development of broadcast antennæ, one question still remains open: is a horizontal antenna which is supplied by power leads at the centre better than a vertical one? According to experiments which were carried out in 1926 in relation to the introduction of horizontal antennæ for the short waves, the horizontal antennæ appeared to be superior to the vertical ones, even in the region of the broadcast wave-length, if they were brought up to a height of $\frac{1}{4}\lambda$ above the ground; and, surprisingly, both in a direction at right angles to the plane of the mast and also in the direction of that plane. On account of the complication in antenna building, no further experiments were carried out with such antennæ. When these experiments with horizontal antennæ are settled—though, indeed, they need not lead to a solution of the antenna problem—we shall have done everything possible as regards broadcast antennæ."

Dr. Meissner informs us that certain German broadcast transmitters use a fundamental wave equal to that of 0·6 of the antenna. The paper shows, however, that this is based more upon the idea of having a high radiation resistance than of producing a stronger ground ray.

Dr. Meissner suggests the use of a horizontal antenna. This should be investigated because it might have great practical value. The investigations on the design of antenna for broadcast transmitters are by no means complete. It is hoped, nevertheless, that the present paper will do something to help the broadcasting wireless engineer in the design of his aerial and the prediction of service area.

* H. M. O'Neil: *Proceedings of the Institute of Radio Engineers*, 1928, vol. 16, p. 873.